

In Charles Ragin and Howard S. Becker, What is a Case? Exploring the Foundations of Social Inquiry. New York and Cambridge: Cambridge University Press. 1992: 173-202.

THEORY ELABORATION:  
THE HEURISTICS OF CASE ANALYSIS

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A colleague once confessed that since getting his degree he seemed "progressively to know more and more about less and less." He referred to his transition from a graduate student required to demonstrate competence in several diverse areas to a professional sociologist who selectively focused his reading and research around a particular interest. Perhaps many sociologists experience this same narrowing of focus. Our early interest, whether our own impassioned choice or fortuitous circumstance, tends to get reinforced as we seek a professional identity. Although our teaching may push us to diversify, and we often create courses for exactly that purpose, most of our teaching innovations harmonize with our research focus. Moreover, once we've given a paper at a professional meeting or published on a topic, other opportunities tend to come to us in the same area. Ties to colleagues with similar interests further reinforce our career path. These professional networks, plus the extensive knowledge accumulated from reading and research experience, bind us to our chosen specialty. To change specialization requires intellectual, professional, and social costs that few are willing to pay. Although over the course of a career many people's interests do change, those shifts tend to be gradual transitions that slowly and subtly rearrange our intellectual, professional, and social commitments (trace, for example, the intellectual course of the major writings of Howard S. Becker and Rosabeth Moss Kanter) rather than sudden, dramatic (and costly) jumps from, say, theories of the state to the dynamics of interpersonal relationships.

Our tendency to stick with a particular problem or field of inquiry and know it both intensively and extensively undeniably expands our ability to develop theory. Yet, ironically, the professional niche we create for ourselves also can constrain theory development. Specialization results in a tendency to tie our research questions to some organizational form that has a particular function: educational institutions, nation states, business organizations, families, elite networks,

social revolutions, communities, courtrooms. But the lack of variation in our choice of organizational form can inhibit the discovery and development of theories, models, and concepts that are broadly applicable. In the sociology of organizations, for example, organization theory is biased by the predominance of research done in hierarchical organizations, generally business firms. How would organization theory change if it were grounded in research that delved into non-profits, worker-managed firms, small organizations, and non-hierarchical organizations to a similar extent? In the sociology of the family, those who study violence typically do not explore violence within and between other organizational forms--delinquent gangs, schools, communities, terrorist groups, prisons, nation states. They strive for a theory of family violence, rather than working toward a general theory of violence. While their selectivity suits their interest and conforms to disciplinary standards for generating formal theory, omitting other types of violence from consideration precludes finding both support for and challenges to their own theories. Moreover, they obviate their own use of theories, models, or concepts particular to violence in these alternative organizational forms that might have explanatory potential in the special case of the family.

Similar examples can be found in every subdiscipline. In political sociology, for example, the concept "prefigurative politics," developed in research on social movement organizations (Breines 1982; Gamson 1991), may have undiscovered utility for the study of social revolutions in nation states. Conversely, theories about social revolutions may lend insight into social movement organizations. This is not to deny the potential of quasi-experimental comparative analysis of comparable organizational forms varying along some dimension for generating formal theory (see, e.g., Przeworski and Teune 1970). Rather, I am suggesting that specialization often results in fixed preconceptions about the organizational form appropriate for case analysis and these preconceptions, in turn, can result in unacknowledged disadvantages for theorizing. By limiting our sociological questions to particular organizational forms, we tend to build on existing theory or generate new theory in fragmented rather than integrative ways.

Breaking away from these preconceptions can stimulate theoretical innovation. If there is a possibility for developing general theories of particular phenomena, it lies not only in acknowledging social organization as a context for behavior, but in empirical examination and comparison of the dependent variable of interest in a variety of organizational forms. As sociologists tend to pose either structural or normative theories to explain behavior (Hechter 1983, pp. 4-6), whenever we do empirical work, we selectively study some aspect or aspects of social organization as it relates to our research problem. Thus our research always can be thought of as organizational, although the form of social

organization, the aspects of it selected for investigation, and the researcher's acknowledgment of the importance of the organizational context vary. It is the researcher who chooses the case--the locus for exploring the research question--and then makes the theoretical case for it (Walton 199x). Since a case is whatever we decide it is (on this the authors in this volume seem to agree), we can vary the organizational settings we select to explore our research questions and systematically assess and compare the findings. Wieviorka, discussing comparative analysis of idiosyncratic cases, states "...a case becomes the opportunity to produce knowledge about how it is both specific to and representative of a larger phenomenon. Its originality does not keep us from making comparisons; and its representativeness does not refer to a metasocial law but to analytical categories" (199x, p. 15).

In this paper, I describe how to elaborate sociological theory by exploring a particular phenomenon in case studies of organizational forms of differing size, complexity, and function and improving/altering theory by alternating between units of analysis. The goal is not theory testing, in the deductive, positivistic sense of the practice, but elaboration of the theory of interest through qualitative case analyses. By theory, I mean theoretical tools in general (theory, models, and concepts) rather than a more restricted formal meaning (a set of interrelated propositions that are testable and explain some phenomenon). By elaboration, I mean the process of refining a theory, model, or concept through qualitative data analysis in order to specify more carefully the circumstances in which it does or does not offer potential for explanation. By cases, I mean organizational forms that are analyzed regarding some similar event, activity, or circumstance: for example, social control in family, nation state, church, county, corporation, professional association, or network. The cases can be ethnographies, analyses based on interviews and documents, or historical comparative studies.

Varying both organizational form and function is crucial to this method. Stinchcombe points out that "lots of facts" are "good hard stones for honing ideas" (1972, p. 5). The transformative powers of this approach lie not only in having lots of facts, but in the radically different kinds of facts that varying cases can produce. Exploring theoretical notions in diverse organizational forms has three major benefits for theory elaboration. First, because shifting units of analysis can produce qualitatively different information, case comparison can generate startling contrasts that allow and, in fact, demand us to discover, to reinterpret, and ultimately to transform our theoretical constructs. Second, selecting cases to vary the organizational form sometimes permits varying the level of analysis. Because of the different sorts of data available from micro- and macro-level analysis, choosing cases that vary both the unit of analysis and the level of analysis,

when possible, can lead to the elaboration of theory that more fully merges micro- and macro- understandings. Third, this method can be particularly advantageous for elaborating theories, models, and concepts focusing on large, complex systems that are difficult to study. Shifting to a different organizational form may create access to data previously unavailable; or it may create a possibility for measurement previously precluded by the size, complexity, or norms of privacy of the organizational form chosen as the research setting. Collins, discussing the world system perspective, asks: if there is only one world system, how can we test a theory (1984, p. 341)? When testing a theory is not possible, perhaps an alternative organizational form can be found to serve as a setting for its heuristic exploration.

I will describe this case-oriented method and its rationale, illustrating its potential for theory elaboration with examples from several well-known sociological studies. I will demonstrate the strategy in greater detail by showing how I elaborated Merton's (1968) Social Structure and Anomie Theory (SSAT) into an explanatory scheme for understanding organizational misconduct. Then I will show how I am elaborating this scheme further through case analyses of misconduct in three organizational forms varying in size, complexity, and function: NASA and the Space Shuttle Challenger accident, police misconduct, and family violence. From the NASA case, I will illustrate how this method resulted in the three benefits to theory elaboration mentioned above. Finally, I will discuss one of the methodological issues that can confound theory elaborated by this method: the problem of forcing fit.

## METHOD

We begin by using a theory, model, or concept in a very loose fashion to guide the research. Cases are chosen because (1) they are potential examples of research topic X, (2) they vary in size and complexity ( e.g., groups, simple formal organizations, complex organizations, subunits within them, or networks), and (3) they vary in function (e.g., accounting department, church, environmentalist group, government, seminar, corporation, research institution, symphony orchestra). We analyze the cases sequentially. We treat each case independently of others, respecting its uniqueness so that the idiosyncratic details can maximize our theoretical insight. As the analysis proceeds, the guiding theoretical notions are assessed in the light of the findings. As in analytic induction (Lindesmith 1947; Cressey 1953; Robinson 1951), the data can contradict or reveal previously unseen inadequacies in the theoretical notions guiding the research, providing a basis for reassessment or rejection; the data can confirm the theory; the data also can force us to create new hypotheses, adding detail to the theory, model, or concept, more fully specifying it. Because more than one theoretical notion may be guiding an analysis, confirmation, fuller specification, and contradiction all may result from one case study. Under

these circumstances, each construct can be elaborated to specify more carefully the circumstances in which it does or does not offer potential for explanation.

Respecting the uniqueness of each case also means allowing the idiosyncratic details to lead us to insights about the case itself. Walton (199x, pp. 5-6) makes the point that the first goal in case analysis is to find out exactly what we have a case of. He notes that we tend to select our cases based upon "typological distinctions" grounded in prior assumptions about what defines a universe of particular cases:

But the presumption is faulty. We do not really know these things at all, we simply make guesses about them--hypotheses. There is nothing wrong with that, provided it is clear that the known universe is an illusion and, with it, that the claim to having a case of something is not supported in any substantial way (199x, p. 6).

Having chosen a case on the basis of certain typological distinctions, we may find we were mistaken. Our data may show that the organizational form we thought was an example of X is not an example of X at all, but something very different. In the event we are surprised in this manner, we may decide to develop a parallel theory of the new dependent variable. In the interest of theory generation, whether that theory is the one that initiated the inquiry or some new theoretical notion that we develop from the data, the integrity of the individual case analysis takes primacy. Future research may prove the case to be a member of a class of similar objects, or it may be that the class has only one member. Whichever is true, identification of the defining patterns of each case is a necessary first step. Once defined for that case, the pattern can be treated as a model. Its relevant features may be found in other cases selected by the same selection criteria that selected that case.

In subsequent case analyses, we use the more fully elaborated theoretical notion (or notions) as a guide. Each succeeding case should be selected with a sensitivity to the idea that varying the organizational setting can lead to new insights: for example, a network theory could be explored by studying an international alliance, but it could also be examined by treating the subunits of a complex organization as a network. In this example, we vary the unit of analysis without necessarily varying the level of analysis. But we can select cases intentionally to vary both, in alternating sequence. Suppose we are interested in the concept of culture, originally developed through studies of societies but more recently applied to business organizations as "organization culture" (see, e.g., Frost et al. 1985). We can alter or enhance what has been learned about culture in these complex organizations by comparing it with analyses of culture in families or in

simple formal organizations (see, e.g., Fine 1987). Then, having elaborated the concept in some organizational form where micro-analysis is possible, we may refine it further by working again at the macro-level, using the adjusted culture concept to guide analysis of a community or a nation-state. What we know about culture (or "organization culture") can be reevaluated in light of the data produced by this latter macro-level iteration.

In each case analysis, we use our theoretical notions as guiding hypotheses, elaborated by what analysis discloses about that particular case and the theory, model, or concept that guided the inquiry. Then, the adjusted theory, model, or concept guides the next case study. In keeping with Kaplan's (1964) warning about "premature closure" and his stress on the importance of "openness of meaning," the theoretical notion or notions continue to serve as guiding hypotheses to be further elaborated in future research. Critical to elaborating theory by this method is the systematic comparison of one case with the next, with explicit attempts to (1) distinguish similarities and differences in findings between cases, and (2) specify the theoretical consequences of the compared findings. These conclusions need to be published with the case analyses. When we do not take this final step, we discourage others from building on what we've done, reducing the method's potential for cumulative theory development. One criterion of scientific knowledge is that it be shared. As important as sharing the knowledge, however, is sharing how that knowledge came to us. As one graduate student attending the Symposium said, "We learn theories, but we don't learn how people theorize."

The need to distinguish similarities and differences and specify the theoretical consequences of case comparisons raises an important point. Organizations (or, organizational forms) do not exist in a vacuum. At the same time that they provide a social structural context for individual behavior, they have a social structural context--an organizational environment--that must be taken into account. If, in focussing on some organizational form, we isolate it from its environment, we greatly restrict the possibility of identifying and understanding similarities and differences between cases. Taking into account the organizational environment in case comparison requires that this somewhat nebulous, possibly all-encompassing concept be grounded in some manageable empirical reality. Always, the researcher carves up social reality in order to study it, but it is the research problem that dictates how the carving is done. Therefore, we selectively can identify and isolate for study the social structural contingencies that seem relevant for the event, activity or circumstance with which we are concerned.

We might incorporate the organizational environment of our research setting by examining social structure at two levels: interactional and contextual (see, e.g., Vaughan 1983). Depending on the organizational form chosen as the setting

for the dependent variable, we may want to examine the immediate social structure, delineated for research purposes by interaction between the organizational form and other organizations acting as competitors, consumers, controllers, or suppliers. In addition, we might identify influential factors in the broader social structural context: the history, politics, economics, and/or culture of the community, region, nation, or global system in which the case occurs. Although the organizational form being analyzed may vary from subunit of a complex organization, to family, to nation state, nested organizational environments can be portioned in this manner. By so doing, we situate our case. Not only does this strategy highlight what might prove to be idiosyncratic explanatory factors, but it also forces us to take into account contingent social relations.

### Rationale

Theory elaboration based upon alternating units of analysis is possible because of the hierarchical nature of organizational forms. Social interaction is the foundation from which group life develops. Interaction produces ordered social relationships that vary in form. The degree of social organization within and between these forms varies in structural complexity, which increases with the number of interrelated roles and statuses. The varieties of group life, treated as Ideal Types, can be conceptualized hierarchically on a continuum according to increasing structural complexity: patterned interactions, groups, simple formal organizations, and complex organizations. When so conceptualized, each form exhibits the characteristics of the simpler form that precedes it and adds to them.

The simplest forms of social organization are patterned interactions: crossing the street, waiting in line, applause at a concert. Emergent groups tend to develop around previous interaction patterns, which provide the basis for further structural differentiation and organizational development. The fact that patterns of interaction exist creates the possibility of group life. From interaction, other organizational forms develop as the basic units around which the major tasks of social life are organized. Groups (i.e., crescive organizations) are distinguished from simple patterns of interaction by common values and norms (which lead to interaction on a regular basis) as well as consciousness of kind. But, in addition, the patterns of interaction are altered by the introduction of structural complexity. Because groups are task-oriented, they are comprised of a number of persons whose interactions are based on a set of interrelated roles and statuses. As a consequence, we note the development of simple division of labor and hierarchy. The family stands as the traditional example of the crescive organization, but all groups exhibit these same characteristics.

The simple formal organization, next on this continuum, exhibits patterned interactions, consciousness of kind,

common values and norms, and, in addition, set goals and formalized rules for achieving these goals. Like a crescive organization, a formal organization has a relatively simple structure and a simple division of labor, but is distinguished by formal goals and rules. Finally, the complex organization bears all the defining characteristics of crescive and formal organizations, but also additional levels of hierarchy, more specialized division of labor, increased tendencies toward centralization, greater degree of formalization, geographic dispersion, and so forth.

A network may develop from linkages between the several organizational forms on this imaginary continuum. Consequently, we may have a network consisting solely of crescive, complex, or simple formal organizations, or some combination. We may have intra-organizational networks, comprised of the subunits of a complex organization or informal group alignments. Some networks may be formalized, some not. Because networks assume myriad forms and develop from a variety of the basic forms of social organization, they do not have a place of their own on this continuum. Nonetheless, they are relevant for this method of theory elaboration, as subsequent examples show.

These forms have elements in common that have implications for the logic of case analysis. Not only do they share aspects of structure, but also they have in common processes that are a natural concomitant of organizational life: conflict, social integration, deviance, cooperation, power, socialization, social control, decision making, social change. Certainly, the variety of organizational forms are not strictly comparable. We know, for example, that as structural complexity increases, the possibilities for internal dynamics also increase (Simmel, Wolff, (trans.), 1950). Differences exist between them, yet they are sufficiently analogous to offer us alternative settings in which to explore a particular phenomenon and compare the findings. Because of the commonalities of structure and process, we may elaborate theory by choosing as the unit of analysis an organizational form that varies in structural complexity from the organizational form about which we began theorizing.

But our application of this method is not restricted to analysis between the organizational forms described above; we can apply it within, as well. For example, some concepts, theories, or models developed to explain relations between an organization and its environment can be examined intra-organizationally and vice versa. Here is where creative conceptualization comes into play in choosing cases as alternative research settings. Consider the work of Miles et al. (1982), who explored two competing theories concerning organizational survival: strategic choice and population ecology. They examined five tobacco companies' responses to the Surgeon General's announcement that cigarette smoking is hazardous to health. Instead of studying five complex organizations facing the same environmental constraint,

as they did, we might explore the efficacy of these same two theories intra-organizationally. Suppose, in the interest of theory elaboration, we choose five internal subunits of a complex organization that face some similar constraint originating from the environment. Now, however, we define the environment as the organization to which the subunits belong. We explore strategic choice and population ecology in a context that would permit interviews and analysis of internal documents. In this setting, we get micro-level data that, when weighed against the original work of Miles et al., may allow us more fully to integrate individual behavior and structural factors in an explanation.

### Historical Precedents

Much of this method of theory elaboration no doubt will sound both familiar and self-contradictory. On the one hand, the examination of data with theory elaboration as the goal is seen by some as a major (if not the major) building block of a positivistic science (see, e.g., Kendall and Lazarsfeld 1950). On the other hand, the ideas of Blumer (1969) and Glaser and Strauss (1967) are evident. But testing and verification of theory in the deductive positivistic tradition are not a part of this procedure. It is grounded in the work of Blumer and Glaser and Strauss, but deviates from their approaches in some important ways.

Blumer (1969) argued for using concepts as sensitizing instruments that could give the user a general sense of reference and guidance in approaching empirical instances. His approach was guided by his belief that "every object of our consideration--be it person, group, institution--has a distinctive and unique character and is imbedded within a context of a similarly distinctive character (1969, p. 148)...we have to accept, develop, and use the distinctive expression in order to detect and study the common" (1969, p. 149). He concluded that using concepts as a reference point with which to assay the empirical world would lead to the improvement and refinement of those concepts, for they would be corrected "in light of stubborn empirical findings" (1969, p. 150). This method of theory elaboration incorporates these Blumerian beliefs, but diverges in its extension to relationships between concepts, as they are framed within theories and/or models.

The work of Glaser and Strauss also figures importantly. Like Glaser and Strauss, this approach relies extensively (but not exclusively) upon qualitative data and constant comparison for theoretical discoveries. Like Glaser and Strauss, it involves alternation of induction and deduction. And it relies upon cases chosen to maximize differences in the contexts of similar phenomena, so that what is common appears more clearly and its relevance to different contexts, its

generalizabilities, can become clear. But it differs from Glaser and Strauss in the following ways. They argue for qualitative case analysis entered into with few if any preconceived theoretical notions. In contrast, this method of elaboration requires comparing data with some sensitizing theory, concept, or model. It rests on the assumption that a researcher never begins with a clean slate. Even when we believe ourselves to be unfettered theoretically, we always begin a research project with an arsenal of preconceived theoretical notions accumulated from our own research, our reading of the work of others, personal experience, literature, and conversations that shape our perceptions and ideas in spite of ourselves. Having once read labeling theory, for example, or DiMaggio and Powell (1983) on institutional isomorphism, we cannot block or isolate these ideas from interpretive use, for they may remain part of our world view, activated unexpectedly in response to situations where, rightly or wrongly, we "see" their applicability. Furthermore, once in touch with our data, we tend early to develop a "theoretical fix:" an explanatory scheme that guides the remainder of the work. Because of both these tendencies, we should identify the theoretical notion or notions that guide our research at the outset, as best we can. By articulating them, we can improve our ability to elaborate theory. Moreover, we can better control biasing influences, as will be discussed later in this paper.

Another difference is with Glaser and Strauss's position that verification and discovery cannot go on simultaneously. Discovery, they state, is precluded by natural tendencies to try to force the data to fit the model being verified. In contrast, this method of elaboration is based on the assumption that discovery and verification can go on simultaneously. The "discovery" of another example of X is "verification." Moreover, within the same case analysis we may discover fit with one theoretical notion, non-fit with another, and some new theory, concept, or model. Although comparison of groups is the foundation of both approaches, here we diverge by comparing (1) the findings from case analyses with theories, models, or concepts in order to subject the latter as well as the former to challenge and change, (2) diverse groups (which Glaser and Strauss suggest only when striving for formal theory), and, whenever possible, (3) findings between levels of analysis. While theory generation is a common goal of both approaches, this method diverges again from Glaser and Strauss by aiming to:

- \*develop concepts, models, or theories whose limits and applicability to various organizational forms become increasingly specified

- \*develop a bridge between our study of behavior in and of small groups, on the one hand, and complex systems, on the other

### The Macro/Micro Connection

This latter goal of forging a link between our understanding of small groups and complex systems may prove to be an advantage of this method that is significant beyond the elaboration of any particular theory, model, or concept that we seek. Individual choice and structure are inextricably related. Hence, our ability to offer a full causal explanation of any phenomenon rests upon exploring the macro/micro connection: what structural factors govern or influence patterns of individual choice, how are those choices constructed, and what are the structural consequences? Although many have recognized the importance of the macro/micro connection (see, e.g., Coleman 1986; Collins 1981; Fine 1988; Giddens 1979; 1984), theory remains bifurcated. We generate either macro- or micro-level explanations, ignoring the critical nexus. Moreover, empirical work follows the same pattern. Instead of research that systematically attempts to link macro-level factors and choices in specific social phenomenon, we tend to dichotomize. Both macro- and micro- get their fair share of attention, but in separate projects, by separate analysts. Those who do join them in empirical work most often do so by theoretical inference: data at one level of analysis are coupled with theoretical speculation about the other. Because the macro/micro connection seldom has been traced empirically, knowledge has remained fragmented.

The macro/micro connection is not an insurmountable problem, and, in fact, may not be a problem at all. It simply may be an artifact of data availability. Sometimes we do not have access to information about individual action and the structural determinants of those actions in the same research project, so we are unable to arrive at integrated explanations. We may be denied formal access to people or records, restricting our inquiry to one level of analysis. Or perhaps necessary people, documents, and records no longer exist or cannot be found. But sometimes what appears to be formally available is unavailable in practice. Even when we do have access to both micro- and macro- level data, often we are prevented from working at both levels simultaneously because doing so is a difficult, unwieldy task. The social historian tracing the evolution of social welfare legislation cannot manage both data demonstrating the influence of a changing political economy and the diaries, biographies, and legislative records that might demonstrate how these influences played themselves out in the lives of decision makers. The field worker investigating the interactional basis of culture in a community or a small group cannot simultaneously account for the structural influences originating in the external environment that shape the evolution of that culture and still craft a clear, incisive micro-analysis.

But data that would allow us to make macro/micro theoretical connections may be unavailable for less obvious

reasons. We develop paradigm preferences that restrict our ability to integrate structure and process in our research. In the same way that our preconceptions of cases appropriate as units of analysis can become fixed, so can our ideas about what constitutes useful information and how to go about getting it. We develop very personal research styles that become comfortable, and while we may get better at finding the information we seek and interpreting what we find, we may be blind to other sorts of information because our ability to see it is undeveloped. We readily "see" either micro- or macro-, but not both. When this is the case, the problem still legitimately can be framed as one of data availability: we tend to define and conceptualize our problems at one or the other level of analysis, making certain data available and not others. Such patterned ways of seeing surely are the product of graduate education (e.g., social psychology and political economy are separated in the curriculum), dichotomous departmental emphasis, specialized journals, and intra-disciplinary reward structures. (Note that exploring this fledgling theory of intra-disciplinary macro/micro disjunction itself requires empirical connection between structural factors and individual choice.) While these professional schisms may enhance our ability to answer questions at either the macro- or micro-level, they can create learned disabilities that constrain us from making macro/micro connections.

If the macro/micro problem is an artifact of data availability, then perhaps this method of theory elaboration is one way of closing the gap. The elaboration of existing theories, models, and concepts is grounded in qualitative data. The data come from analysis of cases chosen for suspected similarities, but their differences are equally important. When we vary cases for size, complexity, and function, data formally unavailable to us in a case study of one organizational form may become available in another. When we sequentially alternate units of analysis so that the level of analysis varies as well, we automatically put ourselves in touch with data and theories at both levels. Moreover, by doing so we are forced to take into account factors that we otherwise might not, given our learned disabilities. We must contend with data that lie outside our individual paradigm preferences in order to explain the event, circumstance, or activity central to the particular case. The result can be a dramatic contrast with other cases, forcing us to amend the guiding theoretical notions in sequential case analyses, incrementally merging macro- and micro-level explanations.

#### Further Methodological Considerations

One limitation of this approach is that it is more oriented toward identifying the presence/absence of factors in different cases than evaluating the relative importance of those factors. But their relative importance can be explored in subsequent research constructed for that purpose. This limitation also can be counted as an advantage, however, for

organizational settings can be selected precisely because the researcher is interested in a particular factor believed to be present. Heimer, for example, is developing a theory of taking responsibility by studying several settings, chosen because they are examples of significant theoretical variations (personal communication, 1990). She first examined taking responsibility in the Norwegian State Oil Company during a crucial transition (Heimer 1987). Because she wanted to look more closely at gender differences in taking responsibility, she next investigated families with babies in neonatal intensive care units, also a crucial transition (Heimer 1988).

Another potential problem is that a case-oriented methodology may give inappropriate weight to cases where an expected factor is not found (Cullen, personal communication, 1989). The risk is to assign these cases a special or idiosyncratic theoretical status when, in fact, they may simply be cases where the probability of the factor's occurrence did not hold. The absence of a factor in a particular case cannot lead us to conclude that overall the factor does not play a significant role in other cases of that category. But its absence still clarifies our thinking by forcing us to acknowledge that the role is that of a sufficient rather than a necessary cause. Another concern about this approach may be the time it requires. One person trying to do a sequential analysis of varying organizational forms may have difficulty completing the work. Most certainly, the time required for field work, a comparative historical study, or documentary analysis of a particular organizational form may vary with the size and complexity of the organizational form serving as the unit of analysis, the scope of the problem, available data, etc. Nonetheless, for many the time required may be prohibitive, or at the least, the prospect of spending years analyzing each case may be daunting, especially for the untenured or those who prefer variety rather than what may be a single career-long enterprise.

One alternative is a team of researchers, each person studying the same theoretical notion in a different organizational setting (see, e.g., Hechter 1983). Or the single researcher may choose a single case, using, for example, a theory of family violence to guide analysis of violence in one other organizational form. Another viable and potentially time-saving method is for the single researcher to use a mix of original work and the published work of others. For example, Stinchcombe (1970) developed a model comprised of seven conditions that determine the degree of dependency of inferiors in different types of organizations. He conceptualized the seven conditions from several research projects he did in the 50's and 60's. In the 1970 article, he used these organizations to demonstrate variability in the conditions, filling in with a few additional examples (personal communication, 1990). He then ranked them by degree of dependency of inferiors (some cases ranked: concentration camps in Nazi Germany; craft production; oligarchic unions; modern armies

in garrison). Then, in this same article, Stinchcombe analyzed the published work of others: Weber's (1924) assessment of class relations on the large landed estates of eastern Germany and Wunderlich's (1961) analysis of post World War I conditions. Guiding his investigation of this alternative organizational form with his model, he achieved an analysis of class relations in landed estates at two time periods.

Although original research (field work, comparative historical studies, or documentary analysis) is primarily what I am suggesting here as the basis for case analysis, the example from Stinchcombe (1970) suggests how the completed research of others might productively be used as cases and incorporated with original work to elaborate theory. Another researcher could now analyze Stinchcombe (1970), exploring the several ranked organizations, first specifying more precisely the variation in size, complexity, and function of the several organizational forms he chose and how the seven conditions vary within and between organizations. The second phase would be original research exploring the degree of dependency of inferiors in other organizational forms that would allow Stinchcombe's model to be considered at the micro-level. But theory can also be elaborated from secondary analysis done in its own right. No doubt many case analyses, using the same theories, models, or concepts, exist that have never been systematically compared. The work of Blau (1964) is exemplary: he began at the micro-level, defining social exchange in intimate dyads, then applied the principles he developed to groups, complex organizations, and some inter-organizational forms. Many others have used these ideas in both micro- and macro-analysis in a variety of organizational settings (e.g., Scanzoni 1972 and Pfeffer and Salancik 1978, respectively). These findings have never been juxtaposed and scrutinized for similarities and differences that would promote elaboration of Blau's original ideas.

#### Elaborating a Concept: Loose Coupling

The concept "loose coupling" has a long history in the literature on complex organizations (Corwin 1976; 1981), beginning with Gouldner (1960), who noted that organizations vary in the extent to which the parts are interdependent. A "loosely coupled system" is a complex organization characterized by a high degree of autonomy among its interdependent parts and isolation between the strata (Corwin 1981, p. 262). Weick (1976) used the concept to explain educational organizations, remarking that because of measurement difficulties, the concept of loose coupling lacks precision, yet still can be used heuristically. Indeed, it has proved to be such a rich heuristic device for understanding complex organizations that the concept has become generic, and like the concepts "proactive" and "reactive," usually is used without citing a source. Yet Weick used the concept to describe a particular type of complex organization;

consequently, the measurement difficulties that Weick described were for that organizational form.

Here is an example of this method's advantages for elaborating theoretical constructs focussing on large, complex systems that are difficult to study. Although Weick used "loose coupling" to highlight what he saw to be unique about educational organizations, the concept could be refined by using it to analyze the relationships of cohabiting couples. Despite vast differences in size and complexity, cohabiting couples and complex organizations have characteristics in common (see also McCall 1970), making the two organizational forms sufficiently analogous that variation in the degree of integration of the parts can be explored in both settings. When we shift from complex organizations to cohabiting couples, we see the advantage that varying the level of analysis produces. We not only have abundant information, but qualitatively different information to contrast with macro-level studies like Weick's original one. By analyzing families, the degree of integration of the "separate parts" of an organization can be studied in greater detail. Multiple methods can be brought to bear on the problem: interviews, observation, lab studies, content analysis of autobiographical accounts. At the micro-level, it is possible to explore not only the extent to which partners in a relationship are interdependent, but also how that interdependence varies by task, and over time. Quantitative analysis also may be incorporated. Through measurement, greater specificity could be attributed to the terms "loosely coupled" and "tightly coupled" by researching cohabiting couples, perhaps leading to greater understanding of what it means to have a loosely coupled or a tightly coupled organizational unit, the range of variation between extremes, and the factors associated with variation.

The more fully elaborated concept could then be reapplied in macro-analysis, examining the relationship between the subunits of another type of complex organization: e.g., an alliance of nation states in an international governing body. Although understanding the phenomenon under study is the immediate target in these various applications of the concept, elaboration of the concept or theory in use is an ongoing part of the research activity and as important a goal as the empirical analysis that calls it into use. Measurement at the macro-level may become possible. Block modeling, the definitional tool developed by White, Boorman, and Breiger (1976), is one possible method for establishing variation in the relation between units at the macro-level. A network of organizations would be loosely coupled if the units had few connections, for example. Perhaps we could identify patterned variations between "tightly coupled" and "loosely coupled" networks, because block modeling might show that some units in the same network are more tightly coupled than others, as revealed by the analysis of links between pairs. Some understanding of the range of connective possibilities might result. Even if macro-level application remains heuristic, more sophisticated insights are possible as

a consequence of previous refinement at the micro-level through research on cohabiting couples.

The next step would be to take whatever new insights are gained by the macro-level application of loose coupling and reconsider the degree of integration in small groups. While greater specificity is one hoped for goal (both in terms of clarification of theoretical notions and the limits of their applicability), greater ambiguity is another. Each case analysis will consist of intricate, interconnected detail, much of it perhaps unexpected. It is the "loose ends," the stuff we neither expect nor can explain, that pushes us toward theoretical breakthroughs. If the guiding theoretical notion truly is used as a sensitizing device, case analyses should raise additional questions relevant to understanding the concept, model, and/or theory being considered: for example, what holds loosely-coupled systems together and how do they manage to function (Corwin 1981)?

#### ELABORATING A THEORY OF ORGANIZATIONAL MISCONDUCT

In developing an explanatory scheme for understanding organizational misconduct, I used the principles of the method of theory elaboration described here for the first time. Automatic and implicit at first, it was not until the manuscript was nearly complete that I could see that I was shifting units of analysis mentally as I theorized, and how that had influenced my development of ideas. I wanted to take what I seemed to be doing implicitly and make it explicit. Also, I wondered if we all intuitively do the same thing when we theorize, and whether something might be gained by being more systematic about it. Since then, I have been using the method in a more conscientious, rigorous way so that I could understand how to use it better and at the same time refine the explanatory scheme. Using the explanatory scheme as a guide, I have continued to explore misconduct with three case studies of organizational forms that vary in size, complexity, and function: NASA and the Space Shuttle Challenger, police misconduct, and family violence. My purpose now is to go beyond the preceding suggestive examples of this method, showing my early use of it and how I am continuing to develop both the method and the explanatory scheme in my current work. The examples that follow are necessarily brief and schematic. The promise of this method is better understood through examining the original texts. Interested readers may follow my first application (Vaughan 1983, pp. 54-104). Those passages show the possibility for confirmation, fuller specification, and contradiction that can result from shifting units of analysis. Also you will find the promised "greater ambiguity:" many new research questions as well as questions about other concepts and theories that subsequently required reinterpretation. For examples of applications of the explanatory scheme to the NASA case study, see Vaughan 1989; 1990.

## Developing the Theory

In 1983, I completed a case study of Medicaid provider fraud in which one organization victimized another. Relying on the findings of the case study and integrating them with existing theory and research on complex organizations and on deviance and social control, I developed a structural explanation of the unlawful behavior of organizations. The major elements of that nascent explanatory scheme were:

1. the competitive environment, which generates pressures upon organizations to violate the law in order to attain goals (1983, pp. 54-66);
2. organizational characteristics (structure, processes, and transactions), which provide opportunities to violate (1983, pp. 67-87); and
3. the regulatory environment, which is affected by the relationship between regulators and those they regulate, frequently minimizing the capacity to control and deter violations, consequently contributing to their occurrence (1983, pp. 88-104).

I argued that each of these three is related to violative behavior, but, more significantly, they are interrelated: misconduct results from the three in combination. Because the competitive environment, organizational characteristics, and the regulatory environment are all necessary to a causal explanation, and because they work together to affect decision making in organizations, they constitute an integrated explanatory scheme for understanding unlawful organizational behavior. Many other theories, models, and concepts appear throughout the text, but these three concepts are the major building blocks.

This theoretical explanation began with my heuristic application of Merton's Social Structure and Anomie Theory (SSAT) (1957; 1968) to business organizations. SSAT, when elaborated, became the linchpin of the explanatory scheme. American society was the organizational setting in which Merton studied individual deviance. While working on the Medicaid provider fraud case study, the data and the literature I was reading kept bringing to mind Merton's notion of "blocked access to legitimate means." Although his conceptualization sought to explain the violations of individuals, a lot of it resonated with what I knew about the violations of business organizations. In fact, because of Merton's emphasis on culturally-approved goals for economic success, the theory seemed more consistently to explain the behavior of organizations than the behavior of individuals. I studied his theory, along with its many applications and assessments, reinterpreting everything I read by mentally substituting organizations as the units of analysis. Applying the theory to

business organizations exposed weaknesses in Merton's conceptualization and at the same time suggested how SSAT might be altered to increase its explanatory potential. The key to this was reconceptualizing both "means" and "ends" as scarce resources for which organizations may compete. This reconceptualization not only did away with the troublesome theoretical and empirical problems associated with "means" and "ends," but allowed the theory to be extended to a variety of organizations, not just business organizations (Vaughan 1983, pp. 55-64). Another significant theoretical result was contradiction of the social class implications in Merton's original exegesis (1983, pp. 54-66; 70-73; 84-87).

Once I had elaborated Merton's theory by considering organizations as the units of analyses, the obvious next step was to reconsider the applicability of Cloward and Ohlin's (1960) Delinquency and Opportunity, a theory proposing the availability of illegitimate opportunity structures as a corrective to SSAT. Like SSAT, Cloward and Ohlin's theory focused on the structural determinants of individual deviance in American society. I reassessed it with organizations in mind. Cloward and Ohlin suggested that Merton's idea of restricted access to legitimate means didn't go far enough; his theory must take into account the availability of illegitimate means, in the form of organized crime. Their work started me thinking about business as well as other organizations as opportunity structures for misconduct; obviously, these were legitimate, not illegitimate, opportunity structures. This realization resulted in my integration of "organizational characteristics" into the explanatory scheme. Considering their theory with organizations in mind generated many new insights: for example, Cloward and Ohlin's ideas on standards for individual success became the basis for articulating how organizational standards for success engender continuous structural strain for all organizations, regardless of size, wealth, age, experience, or previous record (1983, p. 59). Most important, illuminating organizations as legitimate opportunity structures for misconduct contradicted the social class implications of Cloward and Ohlin's original work, confirming my reconceptualization of SSAT.

I grounded my social class argument in the hierarchical nature of organizational forms:

Business organizations are, in a sense, twice stratified. They exist in a hierarchy of organizations, and develop an internal hierarchy of their own. Thus, all social classes are likely to be represented among organization personnel. Given a situation of structural strain, organizations present opportunities for unlawful behavior that are linked to position...thus creating the possibility that 'blue-collar' as well as 'white-collar' employees may engage in 'white-collar crime.' Position,

therefore, may have greater explanatory power than social class in some instances...Merton argues that the social and cultural structure exert differential pressure on the lower class to engage in deviance. A business organization is a mini-society, composed of a membership that may represent all social classes. That one strata of an organization is consistently subject to greater tensions to use innovative but socially proscribed methods of obtaining resources than others is an hypothesis that has yet to be tested. It appears to be an oversimplification, however. The idea that structurally induced motivation to engage in unlawful behavior may vary by subunit of an organization, by position in a subunit, and over time suggests greater variation than Merton originally posited. Differential pressure to engage in misconduct needs to be more fully explored in terms of position in a particular structure (Vaughan 1983, pp. 85-87, but see also pp. 54-66; 70-73).

The theoretical problem remaining to be resolved was patterns of individual choice: why do some who are subjected to competitive pressures and surrounded by opportunities act unlawfully on behalf of an organization while others do not? Since rewards and punishments influence choices people make on behalf of their organizations, the ability of other organizations to impose costs affects the probability that opportunities for misconduct will be used. Thus, the regulatory environment became the final conceptual building block of the explanatory scheme (1983, pp. 88-104).

### Theoretical Gaps

At the time of publication, I believed that my effort to understand misconduct was limited in two major ways. First, while I was aiming for a theoretical explanation of the violative behavior of organizations in general, most of the existing theory and research focussed upon only the violations of corporate profit-seekers. Although I relied heavily upon the more broadly based theory and research on organizational behavior, sociological analysis of violations by organizations other than corporations was scant (but see, e.g., Punch 1985; Sherman 1978). Second, while I believed that the link between individual choice and the structural determinants of those choices was paramount to understanding misconduct (1983, pp. 68-73; 84-87), we knew very little about how structural factors translated into the internal dynamics of organizations and affected decision making. The model most frequently supported in the literature as an explanation of individual action in "white-collar crime" is the "amoral calculator" model: the violating business firm is portrayed as an amoral, profit-seeking organization whose actions are motivated by managers rationally calculating costs and

opportunities (Kagan and Scholtz 1984, pp. 69-72). Research, however, has focussed on structural factors associated with violative behavior, not the decision to violate itself (see, e.g., Staw and Sz wajkowski 1975; Clinard and Yeager 1980). We have not been able to trace the connection between structural factors and individual decisions to violate, so the "amoral calculator" model is untested.

Although some scholars aimed at the connection between macro- and micro-levels of analysis, these attempts mainly were theoretical, empirical work being limited by data problems. Most analysis was retrospective. Data tended to come from enforcement actions, leading to research on the enforcement process itself or restricting the research focus to what data the enforcement process produced about what went on inside the organization. Hence, we seldom had access to information about individual decision making and the structural determinants of those decisions. The work of Geis (1967), Cressey (1953), Quinney (1963), and Sutherland (1949) were the accepted classics that included both micro-and macro-levels of analysis. While social organization was incorporated into these works, their exploration of the connection between the actions of individuals and the formal and complex organizations to which they belonged was extremely limited.

The results of applying theories from one organizational setting to another in case analysis led me to believe that both the ambiguous micro/macro connection and the business firm bias in the explanatory scheme could be corrected by (1) analysis of the violative behavior of organizations other than corporations and (2) employing the case study method in situations where qualitative analysis was most likely to produce new information. I had been teaching an undergraduate course in criminology, in which I taught a unit on corporate crime using the explanatory scheme. The course also included lectures on police misconduct and family violence, and over several semesters I noticed analogous causal factors between these three forms of misconduct. I experimented in the classroom, creating a unit about organizational misconduct using the explanatory scheme as a tool for analyzing all three. The success of this as a teaching strategy and what I was learning from it convinced me that research was the next step. Police misconduct and family violence looked like exciting cases to include, but I wanted a complex organization of another type to replace the often-studied corporate profit-seeker.

#### NASA and the Space Shuttle Challenger

In the early testimony during the 1986 Presidential Commission's investigation of the Challenger tragedy, many of the factors having known association with violative behavior were uncovered. My preliminary analysis, based upon

published accounts and the first volume of the Commission's Report (1986), suggested that internal rules and industry rules were violated in the events leading up to the accident. The NASA case provided an opportunity to move beyond previous understanding because of the unusual data available. First, the case involved the combined activities of a government agency and several private enterprise organizations (e.g., Morton Thiokol, Inc., the manufacturer of the flawed Solid Rocket Booster), a combination providing desirable variation in size, complexity, and function. Second, the investigations of the Presidential Commission and the House Committee on Science and Technology--and the reactions to the event by the media, employees of both NASA and Thiokol, scientific experts, space historians, and others --produced information in abundance.

Much of this information was directly relevant for an organizational analysis: tables of organization, rules and procedures, the history and goals of NASA, and its relations with other organizations (competitors, suppliers, customers, regulators). More to the point, much of the information pertained to NASA's decision making, not only for the Challenger launch, but for previous launches. Here was an opportunity to explore the macro/micro connection in a single case study. Perhaps the case would shed light on the "amoral calculator" hypothesis. The entire verbatim testimony from both official investigations was published (Presidential Commission 1986; U. S. House Committee on Science and Technology 1986). These published volumes contained multiple accounts of decision making, not only in the 24 hours preceding the Challenger launch, but for each launch from the inception of the Space Shuttle Program. In addition, whistle blowers and journalists published accounts with alternative interpretations. The National Archives held some 120,000 pages of documents generated by NASA, contractors, and regulators prior to the accident, plus 160 typed transcripts of interviews conducted by FBI agents after the accident. Over sixty per cent of these interviews were with people who never appeared before either official investigating body.

I began analyzing the various sources, filing information on 4 x 6 cards. To organize the data, I reduced the explanatory scheme to skeletal form, reconstituting it as an analytic framework. These concepts constitute the analytic framework: Environment (competition; scarce resources; norms); Organization Characteristics (structure; processes; transactions); Regulatory Environment (autonomy; interdependence). Note, first, that each of the three major building blocks contains many concepts, theories, and models that have grown out of the literature on deviance and social control and the literature on organizations developed by many scholars over the years. I use these very broad categories, rather than a more detailed organizing schema, in order to maximize discovery. The point of a heuristic device is to sensitize,

to open the researcher to possibility. Beginning with a few major concepts that are provocative and seem typologically distinctive allows us a first rough sorting and sifting of data that illuminates the variation and ambiguities within categories. This rough sorting is then followed by fine tuning at regular intervals in order to elaborate these categories as we go along. The concepts that are not included in the skeletal form of the explanatory scheme remain the subject of inquiry (for a detailed example, see Vaughan 1986, pp. 197-202). Second, not all the theoretical notions that comprise the explanatory scheme can be assessed by every case study, for each empirical investigation will yield insights that inform some but not all. Thus, depending upon the data available from the case being explored, one of the three major components rather than the entire explanatory scheme may be the focus, or even a single concept or model within one of them. Finally, although violative behavior is the criterion I used for selecting the NASA organization for study, deciding whether this is a case of "misconduct," "deviance," "crime," "illegality," "unethical behavior"--or simply "conduct"--awaits full analysis (Walton, 199x).

One of the more significant changes from the NASA case analysis is that I now find "organizational misconduct" a more useful conceptualization than "unlawful organizational behavior" (the term used in my 1983 research). The violation of internal rules and industry rules that occurred in this case study occurred within a constellation of organizational factors, justifying expansion of the conceptual definition to include internal rule violations rather than restricting study to illegalities. The more encompassing concept of organizational misconduct adheres to the principle of beginning broadly to maximize discovery. It promotes contrast and refinement of differences and similarities between behavioral types. The definition of organizational misconduct I am using:

Violation of laws, administrative regulations, or internal or external rules by an act of omission or commission by an individual or group of individuals in their organizational roles acting on behalf of the organization or some subunit of it.<sup>i</sup>

This definition is merely a selection rule to guide my choice of cases for analysis, so is open to alteration or rejection as the findings dictate, in keeping with the goal of elaborating theory through discovery. Although this is no place to discuss in detail the full analysis, here, in brief, are some of the directions in which the original explanatory scheme has been elaborated by analysis of the NASA/Challenger case.

The Competitive Environment. No findings contradict the various theoretical notions included in the original conceptualization. Indeed, the finding that the Space Shuttle Program was born into an environment of scarce resources,

with the burden of carrying out the U.S. government's goals of primacy in the international competition for scientific and military supremacy in space, is strong confirmation for the general relevance for two of the major concepts, competition and scarce resources. The effect of external normative standards pertaining to misconduct in and by organizations remains complex and empirically elusive. Some important unanswered questions: how can we trace the connection between norms external to an organization and the behavior of individual actors? Do organizations create internal normative environments that are distinctive or do they incorporate elements of external normative standards? If the latter, how do we distinguish one from the other? What about intra- and extra-organizational variation in normative standards and how their effectiveness is mediated by many factors, including individual willingness to abide by them?

Organizational Characteristics--Structure, Processes, and Transactions: By virtue of the extraordinary historic documentation of internal NASA affairs, this case produced rarely available micro-level data. Many of the new insights from the study are about what happened intraorganizationally. One example is the elaboration of Spence's (1974) model of market signaling. Spence described how organizations make decisions in a world of incomplete information. He argued that because of the number and complexity of transactions in which organizations engage, and the amount of information necessary to complete each one, they are unable to know each individual case thoroughly. Observation costs are high. As a consequence, organizations tend to use a short-cut assessment method when considering a transaction where product uncertainty exists, relying on signals and indexes rather than bearing the costs of a thorough inquiry. Spence used transactions in the job market as an example: an employer, confronted with a pool of potential employees and unable to gather complete information on each one, relies on indexes and signals, like the prestige of a person's school and/or letters of recommendation. Although Spence's model explained how organizations interpret information originating from individuals, in the 1983 Medicaid provider fraud case I applied his principles to exchanges between organizations. Because of the new information from that case, I was able to see how high observation costs create the possibility of manipulating signals, and thus fraud between organizations (1983, pp. 78-81).

In later research on intimate relationships (Vaughan 1986), the key question was how one person in a relationship could leave the other person socially and psychologically without that other person being aware of it until too late in the uncoupling process. In that research, I had micro-level data. I discovered that individuals in cohabiting intimate relationships (the smallest organizations we create) evolve a method of communicating information that had the

characteristics that Spence originally pointed out and that I had elaborated at a structural level in my previous book. Uncertainty and high observation costs led to a reliance upon signals rather than intensive monitoring of each and every exchange. Again, fraud was a possible consequence. Because analyzing intimate relationships produced a different kind of data, I learned more about the sending and receiving of information: the characteristics of signals sent by unhappy initiators in a troubled relationship and how they were interpreted (or misinterpreted, as was generally the case) by still-contented partners in a dyad (Vaughan 1986, pp. 62-78).

This discovery was truly a surprise. I did not enter into my research on uncoupling with any ideas about the comparability or non-comparability of communication in these two situations, nor did I have the idea at the time that the two research projects would have anything in common at all, other than the fact that both focussed on organizations. Only after I had analyzed the data and written a chapter draft did I see the parallels between Spence's ideas, my research on Medicaid provider fraud, and information exchange in coupled relationships. Indeed, this discovery was what prompted me to begin thinking more systematically about theory elaboration by alternating between units and levels of analysis.

Having applied Spence's model at the macro-level to inter-organizational relations and then at the micro-level to intra-organizational relations, I again was surprised to find the elaborated model applicable in the NASA study. Almost every available source documented that communication problems at NASA contributed to the Challenger tragedy. I did not set out to intentionally apply Spence's elaborated model, but Uncoupling had sensitized me to the importance of the characteristics of signals. So I was curious about information about the flawed solid rocket booster joints, how it was distributed, presented, and interpreted at NASA. The data allowed me to (1) refine my previous notions about the sending and receiving of information in complex organizations, (2) use the elaborated version of Spence's model to explain intra- and inter-organizational communication at NASA, and (3) offer a plausible explanation of why Challenger was launched despite signals of danger. The theoretical significance is that the NASA case provides micro-level data that contradicts the "amoral calculator" model. This finding does not disprove that model, but does challenge it, suggesting the need for more studies with micro-level data about misconduct in organizations.

The Regulatory Environment: While the above allowed me to begin filling a theoretical gap of which I was keenly aware, the data on safety regulation at NASA drew my attention to a subject that hadn't even occurred to me. Theory and research on regulation are based almost exclusively on government agencies regulating business firms. The NASA case

was an instance of a government agency regulating itself and its own product. Moreover, safety at NASA was regulated by one external and two internal regulatory bodies. Consequently, it was a chance to study self-regulation reinforced by external control. Little empirical work exists about the organizational dimensions of self-regulation: internal units authorized by a formal organization to investigate internal affairs in order to control deviant events (for exceptions, see Katz 1977; Punch 1983); the combination of internal and external regulatory units had not been subject to case analysis before. The concepts of autonomy and interdependence, previously used to explain regulatory relations between legally-empowered agents of social control and business firms, guided the analysis. The case study reveals the effects of autonomy and interdependence in the regulation of a government agency. It also shows the applicability of these two concepts intraorganizationally as well as interorganizationally. The application of these two concepts can be broadened as a result of this inquiry (see Vaughan 1990).

#### THE PROBLEM OF FORCING FIT

The paradox of theory is that at the same time it tells us where to look, it can keep us from seeing. Glaser and Strauss argue against initiating qualitative analysis with any preconceived theory that dictates relevancies in concepts and hypotheses prior to the research (1967, p. 33). They warn that proceeding in this manner creates a tendency for a researcher to "force fit" the data to the theory. Moreover, they argue that having a theory in mind inhibits discovery. Because theories, models, or concepts are points of departure in this method of elaboration, does it create a propensity to see a "fit"--or create a "fit"--when none exists? Does approaching a case with a possible explanatory scheme in mind, as suggested here, block discovery of the fresh and new? The argument could be made that this method poses no such danger. Here we use theories, models, and concepts as sensitizing devices, rather than translating them into formalized propositions that are tested; consequently, working within this mode is no different from, say, beginning a study of a prison release program with an array of conceptual tools (e.g., labeling theory, deterrence theory) as a part of our background reading. On the other hand, isn't there a greater tendency for bias when the pre-determined task is to look for, examine, and possibly apply a particular theoretical notion, or assemblage of theoretical notions?

Bias is inherent in both the above situations. Undeniably, theoretical notions affect our interpretation of information, and the information we select to interpret. My affinity for an organizational paradigm, for example, means a particular reading of the data, not the only reading possible. But I am concerned here with unacknowledged biasing effects, which raise the possibility of some distortion being introduced into the work so great as to make it useless or invalid (Becker

1967). The requirement of this method--that, to the best of our ability, we make our theoretical notions explicit from the beginning--creates the possibility of control. We take an intuitive practice--using theories about the world to organize and understand it--and make the practice overt so we can better direct our analysis of social situations. This not only enhances our ability to develop theories, models, and concepts, but also increases the possibility of controlling (although never totally eliminating) bias.

Two safeguards against the unwitting force-fitting of data to theory are built into this method because it is based on case analysis. The cases selected produce unique data that draw the researcher away from the theory, model, or concepts that are guiding the analysis. First, different organizational forms produce variation in the data that exerts a control. Examining organizational forms diverse in size, complexity, and function as opposed to choosing similar organizational forms (studying only families) will lead to information that will not immediately confirm our views. Second, each case has some circumstance, activity, or event that must be explained. Abbott notes the complexity and narrative order of cases, suggesting they "engage in a perpetual dialogue with their environment, a dialogue of action and constraint that we call plot" (199x, p. 18). Each case analysis must have internal integrity, explaining as fully as possible its plot. A full explanation can only be achieved by exploring the relationship between all the parts of the whole (Ragin 1987). Because of this interconnectedness, the researcher cannot isolate a portion of the findings that appear to be an example of a guiding theory or concept without also taking into account the whole. This necessity acts as a check upon unintentional distortion to fit preconceived notions.

The cases will produce new information that does not readily fit the theoretical notions guiding the inquiry. The resolution of contradictions (selective dropping of information that doesn't fit, versus rejecting the theory, versus conceptual innovation) cannot be left to chance, however. The researcher needs to initiate strategies that guard against unacknowledged biases. Of course, reliance on the tools and techniques of the discipline to insure that the research meets the standards of good scientific work is necessary. Moreover, by acknowledging our theoretical tools (i.e., our "biases") as best we can at the outset, we can better guard against the tendency for our world view to affect our interpretation of information in unacknowledged ways. But, in addition, the researcher actively can incorporate strategies into the investigation that specifically monitor the subtle influence of biases by forcing her to consider contradictory points of view.

Systematic generalization

Systematic generalization heightens a researcher's sensitivity to biases in an ongoing work.<sup>ii</sup> It consists of three procedures that make the researcher confront alternative explanations directly and regularly: collegial exchange, using insiders and outsiders, and comparisons with existing documented cases. For systematic generalization to keep the researcher effectively in touch with bias, these procedures should be integrated into the research process at scheduled intervals (hence, "systematic"). The primary purpose is not ultimately to generalize, but to free the researcher's mind both from total preoccupation with the intricacies and influences of the case at hand and to force attention to considerations of broader scope (hence, "generalization").

**Collegial Exchange.** As the data are gathered and analyzed, regularly airing the case analysis with colleagues (preferably in a small seminar environment) as the work progresses subjects the researcher's ongoing mental processes to analysis by others who neither are wedded to the researcher's theoretical viewpoint nor seduced by the particular case that becomes so central to the researcher's life. The subtle acquisition of bias can be exposed in discussion with colleagues who suggest alternative interpretations. For the sociologist working alone, the regular integration of collegial exchange throughout the entire research process is a key mechanism for sensitizing the researcher to unknown bias in interpretation. This may also be an important strategy for a team of field researchers. Though regular exchange among them can provide a check on biasing influences, the group can develop a "theoretical fix" so that they evolve an analysis that reflects the theoretical premises of the group as a whole. The predisposition to fit the data to the theory may be fulfilled unless they seek regular exchange with non-involved sociologists or people from other disciplines.

These conversations also can be important correctives for our understanding of the theories, models, and concepts guiding our work. As Platt and White pointed out during this Symposium, we unintentionally can distort these theoretical notions. From our reading and research experience, we tend (as in all other matters) to remember selectively. We condense our readings of the work of others, remembering main points, forgetting others, perhaps misinterpreting or missing something useful in the process. We can self-correct by re-reading periodically. But our colleagues, perhaps remembering other aspects of a given work or even other relevant works that we've ignored, will remind us of what we've forgotten or never noticed in the first place.

**Using Insiders and Outsiders.** Insiders are participants in the event under study who are interviewed as primary data sources. Chosen because of their involvement in the case under study, their review of the work in progress can correct both factual and interpretive errors. In the NASA study, I circulated early drafts of papers or chapters for comment to

insiders who were primary data sources, which led to both correction and new information. Some of what they said (and equally important, what they didn't say) gave me perspective on the biasing effects of their world view. In order to evaluate insider data, the researcher must not only be informed about the context, but must know the source of the data as thoroughly as possible, must wonder why people agreed to cooperate, must consider how information was selected to give the researcher. Consequently, insider information should be balanced by incorporating the perspectives of outsiders.

Outsiders are individuals informed about the subject matter who, because of position within the group, in another group, ideology, occupation, or even in varied proximity to the event or setting, may have different perspectives than the primary data sources. In examining NASA's regulatory environment, for example, insiders were people who worked in the three safety regulatory units I studied. Outsiders were people who regulated NASA but were not in the three units, journalists who wrote about safety at NASA, "whistle-blowers," and NASA employees who were subject to regulation. Data from outsiders (interviews, internal documents, published accounts) help us know the organizational setting or event from the perspective of others in the environment. Outsiders, of course, have their own biases; consequently, submitting preliminary drafts to them not only can reveal biases in the analysis by forcing us to consider alternative points of view, but also can enlighten us about the biases of these outsiders.

Case Comparisons. Comparing the ongoing case analysis to existing documented cases forces us to maintain a keen sense of the idiosyncratic qualities of the work-in-progress, preventing us from selective attention to data that conforms to our theoretical hunches (Glaser and Strauss 1967). We can quickly acquire comparison cases through historical documents, journalistic accounts, or other written materials by non-sociologists. For example, Phyllis Rose's engaging biographical accounts in Parallel Lives: Five Victorian Marriages (1983) were wonderfully useful for comparison with my interviews for Uncoupling. Another source for comparison cases is systematic analysis of other sociological research guided by a particular concept, theory, or model. Earlier I discussed the role of secondary analysis in theory elaboration, but analyzing the work of others deserves mention again as a bias-reduction strategy: it forces us to confront facts that do not readily fit our preconceptions. Useful for comparison with research on corporate crime, for example, is a collection of 17 cases of government illegality, the data for each case presented in individual chapters (Grabosky 1989). When relying on written materials as comparison cases, however, we must bear in mind that, like ourselves, other people selectively organize information into memory and into documentary form (Smith 1974).

Because data gathering and analysis are simultaneous and we tend to develop hypotheses during all stages of our work, systematic generalization is most effective when regularly integrated into the research process. Of course these suggestions will need to be tempered to fit the problem being studied as well as the number of researchers participating, but frequent direct confrontation with contradictory evidence can monitor bias developing in the research. Careful inspection and record keeping are essential. We tend to forget those bits of information that do not conform to our own world view. With careful inspection and record keeping, we can keep in touch with the idiosyncratic characteristics of the research, reducing the possibility that sensitizing theories, concepts, or models will lead to dropping of information that does not fit. In this way, we can maximize the heuristics of case analysis.

## NOTES

1. This definition is a variant of Shrager and Short's definition (1978). I have eliminated their reference to social and physical harm and added the violation of internal rules.
2. An earlier version of these ideas appears in Vaughan 1983: 133-135.

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